Peer-review report of

Kukharkin, A., Barber, B., Cooley, E., Caluori, N., Brown, X., Singh, A., Cipolli, W. & Brown-Iannuzzi, J.L. (2026). White Americans' feelings of being "last place" are associated with anti-DEI attitudes, Trump support, and Trump vote during the 2024 U.S. presidential election. *advances.in/psychology, 1*, e549398. https://doi.org/10.56296/aip00046

Round 1

Editor Decision

Dear Authors,

Thank you for submitting your manuscript, "A Longitudinal Election Study: Non-Hispanic White Americans' Feelings of Being in 'Last Place' Predict anti-DEI Attitudes and Trump Support During the 2024 U.S. Presidential Election" to the special issue on the "Psychology of Pushback." Two experts, well acquainted with your methods and theoretical framework, reviewed your work, and we evaluated the paper independently before consulting their reports. We thank them both for their detailed evaluation.

We recognize several strengths in your research and manuscript. The study involves longitudinal data from a pivotal social period on an important topic. It employs sophisticated analyses, and the manuscript is clearly written. The findings are informative. Reviewer 1 writes, "This is an ambitious and methodologically rigorous study that offers timely insights into the psychological roots of political extremism, right-wing support, and opposition to DEI efforts." Reviewer 2 describes the manuscript as "a really interesting paper with great longitudinal data on a highly relevant topic." We find your work to be stimulating and promising, and we invite you to revise the manuscript for further consideration for publication.

Both reviewers identify areas that could further strengthen the contribution. Because their comments are already detailed and constructive, we will not reiterate them all here. Instead, we highlight a few of the main points and offer comments based on our own independent reading and review of the manuscript. In your revision, please address each of the points raised in this letter and in the separate reviews and explain the changes in your response letter, citing the relevant excerpts.

We present what we view as the most central issues by section below.

INTRODUCTION

Both reviewers identify ways in which the Introduction should be revised to better prepare readers for the method and results and to tighten the rationale to make the new theoretical contribution of the research (e.g., over and above Cooley et al.,

2024) clearer and more persuasive. Reviewer 2's points 1-3 explain changes that will help improve the Introduction. Reviewer 1 recommends, which we strongly endorse, expanding your consideration of potential underlying mechanisms. Reviewer 1 notes that "the term 'alt-right' is politically loaded. A short explanation of how the authors define the boundaries of alt-right beliefs versus mainstream conservatism would help here. In addition, Reviewer 1 requests more consideration of the potentially different impacts of intragroup and intergroup comparisons. Reviewer 1 further comments, "It would help if the authors clarified whether the "last place" effect is mainly about threat (concern over outgroup progress), deprivation (within-group comparison), or a mix of both. This distinction isn't just a matter of theoretical tidiness—it really influences how we make sense of what's driving anti-DEI attitudes and support for Trump…. Clarifying this emotional undercurrent could help the reader better grasp the psychology behind these findings. We believe that deeper consideration of the mechanisms underlying the hypothesized effects will improve the theoretical impact of your work and distinguish the current work from recently published papers on this topic.

We also recommend revisions to the Introduction to better prepare readers for analytic aspects of the manuscript. A central aim of the work is to test the stability of correlations over time, yet the Introduction devotes little attention to this issue. Readers need to understand why meaningful shifts in correlations might appear over such a short interval—otherwise, the rationale for the longitudinal analysis is unclear. Two opposing forces merit discussion: Associations generally weaken as temporal distance increases, yet the growing social tension during the study period could generate a countervailing dynamic.

In addition, the Introduction should better prepare readers for the chosen analytic approach (LPA). For example, given the description of the main predictor in the study, readers might expect some sort of curvilinear test of differences based on where participants place White Americans relative to other racial/ethnic groups. So, it is important to prepare readers for the analytic strategy you adopt and why it is most preferable. Please explain why LPA is particularly well suited to the research questions, for example compared with alternatives such as interaction analyses or RICLPM.

METHOD

Please relocate all descriptions of the analytic approach from the Results section to an Analyses subsection in the Methods.

Please report Cronbach's alphas for each scale in the text and note whether factor analyses support distinguishing the scales, given their conceptual overlap. If not, consider margining some of the scales.

Attrition was an issue of concern to those of us who reviewed your manuscript, and it will likely be an issue for readers more generally. We support Reviewer 1's recommendation to take "a closer look at attrition based on key psychological variables like 'last place' status would help confirm that results aren't biased."

Reviewer 1 adds, “The use of listwise deletion may also limit power or skew results if the data aren’t missing completely at random. A short discussion of this, and possibly a note on more robust alternatives like multiple imputation or FIML, would help underscore the study’s methodological care.” While we are fine with you retaining listwise deletion for your primary analyses, we ask that you consider Reviewer 1’s analytic alternative in supplementary analyses.

Also, please indicate how many people failed the attention checks.

Like one of the reviewers, we were unsure about several aspects of the LPA and the subsequent tests. First, it is unclear whether the LPA used data from one wave or both: the text suggests wave 1 only, yet participants had to complete both waves to be included. Second, by overlooking possible shifts in profile membership over time, the analysis misses an important opportunity. Identifying such changes would markedly strengthen the paper. At minimum, please demonstrate that profile membership is highly stable across waves to justify treating it as a static between-participant variable, and discuss whether the sample size is indeed too small for a three-profile LPA at later time points (as standards differ and your solution is relatively simple; the required N might be satisfied).

As another reviewer noted, it is not evident whether you accounted for classification uncertainty. If you employed a standard *lmer* model, as the use of *emmeans* implies, this uncertainty was probably ignored. Please weight the results—following the method the reviewer outlined or an alternative—to incorporate it.

Consistent with the reviewer’s query, clarify how closely the sample met the quota targets and whether that representativeness changed across waves.

Finally, please move the information now contained in Table 1 into prose within the new Analyses subsection, eliminating the table.

RESULTS

We strongly support Reviewer 2’s (points 5, 7, and 8) recommendations for presenting more details about the results of your analyses.

We also have additional recommendations. In Figure 3, please add a panel showing the actual inequality distribution from available statistics; this will reveal how closely each profile mirrors reality.

In the same figure, graph 95% CIs—these are essential for interpreting the profile differences discussed on pp. 19–20. Also provide formal tests of within- and between-profile group differences (including the self). More broadly, replace all standard errors in tables, figures, and text with 95 % confidence intervals.

When interpreting results, situate the last-place cluster’s scores within the scale format; they often lie nearer the midpoint than at the extremes that indicate clear support or opposition to a given policy or leader. That might be an important nuance to convey and to understand “who they are.”

For clarity, graph the *emmeans* results (with 95 % confidence intervals) rather than listing them in tables.

Figure 4 largely duplicates Table 8. Please integrate the Ns into the figure or its note.

As Reviewer 1 suggested, it would be valuable to examine the relative effects of between-group versus within-group disadvantage. Please consider such a complementary analysis within each profile.

DISCUSSION

The Discussion is clearly written and considers many questions that are likely to arise among readers of this work. However, we also have a few suggestions and requests for this section. While you consider the issue of direction of causality, like Reviewer 1 we ask that you present more concrete guidance about how this could be tested in future research.

Like Reviewer 2 (point 6), we believe that a more direct case for the novel theoretical contribution of the research would strengthen the manuscript.

In addition, it is not clear why you claim there is “especially stable last-place profile.” Please clarify how this was reflected in the results – how was stability tested?

STYLISTIC

Insert all figures as high-resolution SVG files.

Avoid contractions (e.g., “they’re”).

Resolve APA 7 inconsistencies: use “and” rather than “&” in narrative citations (e.g., “Bobo and Hutchings”) and cite as “Cooley et al. (2024)” rather than “Cooley et al., 2024.”

In conclusion, we appreciate your submission of this important work to our special issue. Like the reviewers, we see great potential – practical, empirical, and theoretical –but its contribution is not yet fully realized. As described in this letter, there are several ways the manuscript can be strengthened. The changes we are seeking involve rewriting and some additional analyses, but do not require the inclusion of a new study or data. Thus, as noted earlier, we are offering you the opportunity to revise and resubmit the work for further consideration. While we cannot guarantee the outcome of that process, we would not offer this opportunity unless we believed that it is possible that, with appropriate revision, the manuscript could eventually merit publication. That determination, of course, would depend on how well the revised manuscript addresses the concerns raised.

Given the nature of the revisions we are requesting, we ask that you submit a revised version within 6 weeks’ time (i.e., by September 18). Please outline in a revision letter how you addressed each point and provide excerpts that outline these changes.

Thank you for again for pursuing *advances.in/psychology* as an outlet for your research, and we hope that you view our comments to be constructive, as intended. We look forward to seeing the next version of the manuscript in the not-too-distant future.

Best,

Jonas R. Kunst & John F. Dovidio

Reviewer 1

This is a peer review for **A Longitudinal Election Study: Non-Hispanic White Americans' Feelings of Being in "Last Place" Predict anti-DEI Attitudes and Trump Support During the 2024 U.S. Presidential Election** under consideration by Advances in Psychology.

This is a really interesting paper with great longitudinal data on a highly relevant topic. The paper presents a striking case: due to stereotypes of White Americans being wealthy, some White Americans may feel like they are falling behind economically (both as compared to other White Americans and as compared to Black/Latinx Americans), which, in turn, can cause them to showcase more right-wing extremism including increased likelihood of voting for Trump. This is concerning, especially given the racial wealth inequality in the U.S. typically favoring White Americans. The paper is well-structured, and the arguments are compelling. To test the main ideas, the paper utilizes longitudinal data around the 2024 U.S. Presidential Elections collected in five waves spanning three months between September and November 2024. In the Discussion, the authors thoughtfully reflect on the study's design and data, and they point out key limitations (e.g., the correlational nature of the findings despite the longitudinal design).

I have a few questions and comments which I hope will further strengthen the manuscript:

1. The opening paragraphs are well structured and easy to follow. However, the main point, in my view, from line 66-68, lacks explanation. Can the authors explain in one to two sentences the main mechanisms as presented in Caluori et al. (2024)?

2. Bringing up the emotional and health outcomes of previous studies on page 4 comes as somewhat surprising. How are these relevant for the political outcomes studied in this paper? This leads the reader to speculate: Are they mediators later? Since they're not, I would tone this down/make clearer it is not part of the current study to not confuse the reader.

3. On page 6, the concept of alt-right is introduced. In my view, the phenomenon could be better defined/explained. Similarly, different terminology is used throughout the paper: extremist right-wing, alt-right, anti-DEI, pro-Trump, etc. Please streamline or, if there is a need to use multiple, highlight the subtle differences between the terms. Although similar, they are not the same. In that same paragraph, it also is unclear to me how “falling in last place” would relate to anti-government beliefs. The underlying mechanisms here can be better described.

4. On page 10, line 207, could the authors specify how many participants were retained? In addition, “participants who completed at least two waves were retained” but the LPA was based on the answers given at wave 1. I think that means that all participants have participated in wave 1 and additionally in at least one more wave which or may not be wave 2? On line 215, the authors describe that participants took part in the survey at wave 1 and 2, but what about the other waves? All these parts can be clarified.

5. Regarding selecting the best solution in terms of number of profiles, I think footnote 3 should be part of the main text. I’d like to see a more in-depth description of the overlap between the profiles in the 4-profile solution as compared to the more parsimonious (but else, worse fitting) 3-profile solution. Particularly, the smallest profile in the 3-profile solution is 11%, just 1% above the threshold that the authors have referenced, whereas the smallest profile in the 4-profile solution is 15%. In sum, the 4-profile solution seems a good one to me, so I’d like to read more convincing arguments for why the 3-profile solution was chosen. Specifically, it would be helpful to know exactly what is similar between the 3-profile and 4-profile solution. Moreover, some reflection on the fact that the entropy wasn’t too high in either solution could be good. Did the authors test the structural model with the 4-profile solution perhaps? If so, what was found?

Next to this, the LPA is exploratory in nature, starting with one profile moving to more profiles. As such, if the authors merely wished to replicate previous studies (as outlined in footnote 3, “Given that two prior representative samples, sampling the same population, (see Cooley et al., 2024) settled on a 3-profile model, we reasoned that the 3-profile model elicited profiles that were most interpretable and consistent with prior work“), I am not sure an LPA is the most suitable approach. I would either go with which solution arises from these collected data (fine if this the 3-profile solution given the theoretical overlap that is to be described more in-depth) or predefine categories based on existing work with similar data and simply assign participants a profile based on their scores. Now the arguments are mixed up.

6. It is helpful that the authors list which outcomes are new on page 20, but I think such claims are better in the introduction. Before reading which outcomes were new and the abundance of references to existing work indicating that the results of this study are in line therewith did make me wonder about the novelty of the whole endeavor. As such, it might be best to early-on outline which outcomes are new and what is different from existing work in this realm.

Similarly, the Discussion is largely structured around highlighting what is different/similar to a particular previous paper (e.g., Cooley et al., 2024) but what are the implications beyond this comparison?

7. I am missing information on how the profiles were used to predict outcomes. Was this the distal outcome approach (see Mplus user guide)? How did the authors account for the uncertainty around profile membership that LPA with distal outcomes in Mplus elegantly accounts for when using the profile membership to predict outcomes? Which data were exported from Mplus? In the OSF, I can only find the R code after the profiles have already been made.

8. There is some evidence that the measure used (e.g., slider scale, writing down percentage; Eriksson & Simpson, 2012) influences how participants answer their questions. Can the authors reflect on how methodological decisions influence their results? In the same line, it is interesting that the White Americans in the “last place profile” are objectively best off as compared to the other two profiles, as noted by the authors. The discussion on what this means may be expanded.

Eriksson, K., & Simpson, B. (2012). What do Americans know about inequality? It depends on how you ask them. *Judgment and Decision Making, 7*(6), 741-745.

Minor points:

1. I’d consider removing the reference in the Abstract

2. Festinger (1954) may be foundational, but it could be helpful to also include more recent work that builds on or updates the ideas.

3. Although I agree the entire case is indeed ironic, the use of the word ironic twice on one page is a bit much. I would consider rephrasing either one (line 71 or 90).

4. The anti-DEI measure is referred to using different terms (attitudes toward DEI, DEI ban support, etc.). It may be good to streamline.

Reviewer 2

I have been asked to review the manuscript titled “A Longitudinal Election Study: Non-Hispanic White Americans’ Feelings of Being in “LastPlace” Predict anti-DEI Attitudes and Trump Support During the 2024 U.S. Presidential Election,” which explores the psychological underpinnings of political attitudes among White Americans.

This is an ambitious and methodologically rigorous study that offers timely insights into the psychological roots of political extremism, right-wing support, and opposition to DEI efforts among non-Hispanic White Americans. Using a five-wave longitudinal design and latent profile analysis (LPA) to map subjective status perceptions, the authors present a compelling narrative about how feeling in “last place” predicts support for alt-right ideology, resistance to DEI, and pro-Trump attitudes, both in terms of beliefs and behaviors.

The manuscript is clearly written, and well-structured. It builds meaningfully on earlier work by Cooley et al. (2024) and extends those ideas into the context of the 2024 U.S. presidential election. The replication strategy, high-quality data, and transparent reporting are all commendable.

That said, I have several comments related to theory, measurement, and interpretation, especially from the perspective of intergroup relations, threat perception, and political psychology.

While the study refers to both relative deprivation and status threat, these frameworks are not fully unpacked in the introduction. The discussion would benefit from a clearer explanation of how perceptions of falling behind, either within one's own group or relative to other groups, translate into alt-right sympathies or DEI opposition. The role of group-based deprivation (Kunst & Obaidi, 2020) and zero-sum racial beliefs (Norton & Sommers, 2011) could also be more fully integrated into the argument.

One issue that could use clarification is the proposed psychological mechanism. The authors draw on both group threat theory (e.g., Blumer, 1958; Bobo & Hutchings, 1996) and relative deprivation theory (Smith et al., 2011; Kunst & Obaidi, 2020), but it’s not entirely clear how the two are theoretically distinguished or combined here. These frameworks have important differences: group threat emphasizes perceived competition with outgroups over resources or symbolic standing, while relative deprivation focuses on subjective feelings of disadvantage, even if someone is objectively doing fine.

The authors suggest that today’s economic climate might make some White Americans feel like they’re falling behind their White peers or that they’re being passed over as racial minorities make progress. That’s a really important insight. But as it’s currently written, it blends two distinct psychological ideas: intergroup threat, where people fear losing status to another group, and intragroup deprivation, where

the frustration stems from seeing others within one's own group doing better. It would help if the authors clarified whether the "last place" effect is mainly about threat (concern over outgroup progress), deprivation (within-group comparison), or a mix of both.

This distinction isn't just a matter of theoretical tidiness—it really influences how we make sense of what's driving anti-DEI attitudes and support for Trump. Is it mainly about fear that other racial groups are catching up or getting ahead? Or is it more about a deeper frustration, a feeling of being left behind or invisible within one's own group? The work by Kraus et al. (2017) and Kunst & Obaidi (2020) seems to point more toward this second possibility: that it's not only the comparisons between groups that matter, but also the sense of unfairness or loss when someone feels they're not living up to what they believe they should have, especially when they see others, perhaps those they identify with, doing better. Clarifying this emotional undercurrent could help the reader better grasp the psychology behind these findings.

The idea of "last place aversion" is powerful and well-articulated. Still, psychological research suggests that intragroup and intergroup comparisons don't necessarily have the same impact. Studies on fraternal deprivation and threat suggest that comparisons with outgroups are more likely to trigger strong emotions and behaviors, like resentment, anger, or support for extreme candidates (e.g., Runciman, 1966; Wildschut & Insko, 2007).

While the authors' typology covers both forms of comparison, it might be worth considering whether intergroup deprivation plays a larger role in driving outcomes like alt-right support and Trump loyalty. Could their LPA profiles be used to explore whether between-group status concerns are more predictive than within-group ones?

The authors use two validated scales to measure alt-right ideology (Cooley et al., 2024; Kamenowski et al., 2021), which tap into anti-government, racial, and anti-immigrant attitudes. Still, the term "alt-right" is politically loaded. A short explanation of how the authors define the boundaries of alt-right beliefs versus mainstream conservatism would help here. It might also be valuable to break the alt-right measure into subcomponents (e.g., racial resentment, authoritarianism) to see which ones are most closely linked to "last place" status.

The longitudinal design is a key strength. But the gap between waves is quite short, especially for examining changes in ideology or status perceptions. The authors should briefly discuss what kinds of psychological or attitudinal changes are realistic in that time frame, and which ones likely aren't.

The authors also note that engagement with Trump or alt-right ideology could shape perceptions of status—that is, the causality could go the other way. While this can't be tested with the current data, a brief reflection on how future research (e.g., cross-lagged panel designs or experiments) could address this would strengthen the paper (see Kunst et al., 2019).

A final point concerns the attrition rate. While that's not unusual in online panels, it's notable given the short time gap. The authors say dropouts and completers didn't

differ much on demographics or Trump support, which is reassuring. Still, a closer look at attrition based on key psychological variables like “last place” status would help confirm that results aren’t biased. The use of listwise deletion may also limit power or skew results if the data aren’t missing completely at random. A short discussion of this, and possibly a note on more robust alternatives like multiple imputation or FIML, would help underscore the study’s methodological care.

I hope the authors find these comments helpful, and I look forward to seeing this important work published. Thank you for the opportunity to review it.

Round 2

Editor Decision

Dear Authors,

Thank you for submitting your revised manuscript, “A Longitudinal Election Study: Non-Hispanic White Americans’ Feelings of Being in ‘Last Place’ Predict anti-DEI Attitudes and Trump Support During the 2024 U.S. Presidential Election” to the special issue on the “Psychology of Pushback.” The same two experts have again reviewed your work, and we have evaluated the paper independently, as before.

As you will see, the reviewers are generally satisfied with your revisions. We all applaud the very detailed and rigorous way you considered each of our points and explained the rationale for your changes in the manuscript. For instance, your introduction has become very strong. We also liked the transparency you provided in terms of various methodological details. As a result of your diligent work, the paper has largely improved.

Building on this excellent progress, we believe a few key revisions are needed to make the paper as impactful as possible. Our main feedback concerns your modelling approach, where we see several challenges in the current narrative that we ask you to address.

1. First, thank you for providing more information about why you retained the 3-profile solution. As part of your explanation, you write the following: “Notably, although the 4-profile model yielded slightly higher entropy and better fit by most metrics, the 4th profile was remarkably similar to an existing profile in the 3-profile model—so much so, that from a theory-standpoint, the 4th profile was not differentiable in meaning from the existing profiles in the 3-profile model (see Supplemental Materials a visual of the 4-profile model).” We find that the new profile in this 4-profile solution is the only one where White people have a seemingly large difference from everyone else and thus appears very distinct from the other three profiles. This seems to be also a potentially theoretically meaningful profile that also could bolster the novelty of the present work. It is also not clear why the fact that two prior studies found three profiles should influence the profiling decision, as this

seems to negate the data-driven approach. If replication is the goal, as you state repeatedly, then the decision needs to be anchored in the same fit-based model selection criteria used in those studies. Our understanding from the 2024 paper is that the 3-profile solution also did not have the best fit in this work and that you focused on entropy values and a minimum of 10% in each profile – if you would follow this rule, the 4-profile solution should clearly be selected in the current study (in the 2024 paper, the authors also consider relative improvements in fit; the improvement in the present work stat flattening out after the 4-profile solution, not after the 3-profile solution). Furthermore, it remains unclear what theoretical reason you had for selecting three and not four profiles. **Thus, the solution to this issue would be opt for the 4-profile solution.** We understand that this solution means reanalyzing parts of the data, but the last place profile still emerges and many findings would likely remain the same.

2. The second larger issue regards how you describe the 'last place' profile. First, thank you for providing error bars for Figure 2 and the pairwise comparisons. As the figure suggests and your comparisons show, in the 'last place' profile, there is no significant difference in ratings of the self and Black and Latinx people. This is a significant observation, and we ask that you describe the 'last place' profile accordingly. For instance, you write, “The last place profile appears in Figure 2c, and accounts for 11% of our sample. In this profile, participants see themselves as behind other White people but also see themselves as falling behind all other racial groups”. This description is thus not accurate. These participants see themselves in the last place *together with* two historically disadvantaged groups. This does not make the findings less interesting, but it means you need to carefully revise all parts of your manuscript (including the abstract) and reframe discussions to reflect this nuance. Also, given that these group comparison results are very important, please report them in the main text. Finally, discuss whether this aligns or differs from the 2024 study. Judging by the means reported there, it seems to potentially differ.

3. Third, thank you for ensuring that the main pattern of results was the same when using probability scores. This significantly strengthens confidence in the results. However, the entropy of .76 is not particularly high, and ensuring analytic comparability with a previous study that did not account for classification uncertainty (which is not an optimal analytic approach) is not a principled reason for your choice. Therefore, please either (a) make these probability scores the main analysis, as one of the reviewers suggested, and move the current analysis to the supplemental materials, or (b) add a robustness test section to the main text where you present the probability score results. In any case, please make sure to discuss these results and the extent to which they converge in the discussion section.

In addition to these larger points, please address several other issues.

Table 5 and 7 are helpful. Please present exact *p*-values for the comparisons including effect sizes in addition in the text.

Thank you for providing further information on missing as random; this is valuable information. Please provide statistics from the “multiple logistic regression to assess

how missingness on these variables was related to observed data" in the main text, as this is essential information.

We also ask that you move the two sections you added to the Supplemental Materials, starting with "Initially, when designing our study, we had hoped to…", to the main text.

Finally, as you note, the stability is only moderate-to-good, depending on the measure. It is important to discuss this as a limitation, while at the same time, you could possibly make the point that self-placement was relatively stable.

We also noted several stylistic and formatting issues. The placement of "consequently" is odd in this sentence: "Finally, it is an open question as to how subjective perceptions of being 'last place' may relate to consequently political outcomes over time, and during times of political transitions, such as a U.S. presidential election".

Thank you for providing most figures as SVG files; however, please reconstruct Figure 1 in PowerPoint or a similar program, export it as an SVG, and insert it into the document.

Please correct the typo "Ktiely" to "Kteily".

Table 1 also needs to be formatted according to APA style.

Lastly, regarding the citation check, despite some false positives in our report (https://drive.google.com/file/d/1JmVPFMqRRH6hLoabY5MwKqjzvgA5apnz/view?usp=sharing), there are still several issues (e.g., wrong years, uncited work in the reference list, missing author names) in your citations and references. Please address each of these points **thoroughly** in your revision. There should be no remaining issues of this sort in the next version.

Please move the IRB statement to the methods.

There is a missing word on line 283 (likely "were").

On line 424, the is one extra word: "only."

Please specify the item number for this measure: Opinions on Right and Left Movements.

Please avoid interpreting statistically non-significant findings as "descriptively" aligned with your predictions or the 2024 study.

You vary in whether you "explore" or test hypotheses. It seems as if the latter is the case. Please avoid the term explor* for this reason.

Thank you again for pursuing advances.in/psychology as an outlet for your research. We want to reiterate that we clearly see the value in this research. While we recognize these requests are substantial, our goal is not to create unnecessary hurdles for you. Rather, we are invested in making sure this important work realizes

its maximum impact. We hope you view our comments as constructive and collaborative toward that shared goal.

Given the nature of the revisions we are requesting, we ask that you submit a revised version within 8 weeks' time (i.e., by December 18) and grant you 1,000 additional words. Please outline in an accompanying revision letter how you addressed each point and provide excerpts that illustrate these changes. Please do not hesitate to reach out if you want to discuss potential solutions to the three core issues listed in the beginning of this letter.

We look forward to seeing the next version of the manuscript in the not-too-distant future.

Best,

Jonas R. Kunst & John F. Dovidio

Reviewer 1

This is a peer review for **A Longitudinal Election Study: Non-Hispanic White Americans' Feelings of Being in "Last Place" Predict anti-DEI Attitudes and Trump Support During the 2024 U.S. Presidential Election** under consideration by Advances in Psychology.

I thank the authors for their extensive work with my questions and comments. I also appreciate the visualization of the four-profile solution in the SI.

I have one minor point left regarding the new text as introduced on p. 6 "(…) radical candidate who focuses on (…)". I would remove "radical" as this is yet other terminology that may need explanation (i.e., hints toward radical-right but does not directly tap into it) and may yet mean something different than "alt-right". The sentence will convey the message without "radical" equally well. The other streamlining between terminology and defining core concepts was very well done.

Moreover, I still wonder why the authors report the results based on the classify-analyze approach rather than one of the, in my opinion, more sophisticated and nuanced approaches taking class uncertainty into account. Ideally, I'd switch this around and report the results taking class uncertainty into account in the main manuscript and the other results in the SI. I am glad to know the results weren't markedly different (line 416-420), but it would be an asset to write a few more

sentences in the manuscript about differences in results, if the authors wish to keep the classify-analyze results in the main text.

Else, I believe the authors have sufficiently addressed all my concerns, and the work is suitable for publication in Advances in Psychology.

Reviewer 2

I would like to sincerely thank the authors for their thoughtful and careful revision. It is clear that they have engaged deeply with the feedback and taken the time to address the comments in a thorough and constructive way.

I find the responses and revisions convincing and of high quality, and I appreciate the care that has gone into improving the manuscript. I have no further substantive comments and am pleased with the current version. I look forward to seeing the paper published.

Round 3

Editor Decision

Dear Authors,

Thank you for submitting your revised manuscript to the special issue on the "Psychology of Pushback." We appreciate your constructive engagement with our previous feedback. We are impressed by the new version of your work, which is argumentatively comprehensive yet cohesive, as well as detailed and rigorous in its analyses.

We are particularly grateful for your thorough investigation of the 4 vs. 3 profiling solution issue and for addressing our remaining concerns.

The paper is now very close to acceptance. We request only a few very minor revisions:

1. The title is currently very long, which may reduce its discoverability in databases. We understand this length was intended to address some of the profiling issues. However, because your abstract already provides sufficient nuance, we suggest the following shortened version, which also avoids causal claims (as "predict" may be understood as such): "White Americans' Feelings of Being "Last Place" are Associated With anti-DEI Attitudes, Trump Support, and Trump Vote During the 2024 U.S. Presidential Election"
2. Please replace Standard Errors with 95% CI where applicable (e.g., Tables 4 through 9). Additionally, to ensure the tables are self-contained and self-explanatory, please clarify in the captions or notes that Kaminski and Cooley refer to the *measures* used, not to the results from those specific studies.

3. Please explain what cutoff was used when “iteratively setting the probability to “1” of falling in each profile.” We assume you simply assigned membership when the probability had the highest value within a person?
4. There is an extra word (“our”) on line 682. There are also two extra words (“each profile”) on line 842.
5. Your descriptions in the Notes suggest that different labelling conventions were used in Tables 5 and 8. Specifically, in Table 5, you state: “When means share a letter then these two means do not significantly differ from one another. When means do not share a letter then these two means significantly differ from one another.” In Table 8, you state: “The letter next to the estimated marginal means indicate which means are significantly different ($p < .05$) from one another.” However, it appears you followed the same convention in both. Please ensure the notes describe both tables consistently and that the same convention is used.
6. It is unclear why “*within-* and *between-group*” are italicized throughout the text. We suggest using italics only for the most important emphases.

Given the minor nature of these requests, we ask that you submit a revised version within two weeks (i.e., by December 20). Please outline in an accompanying revision letter how you addressed each point.

Best,

Jonas R. Kunst & John F. Dovidio